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(1) '*ON THE ELECTRIFICATION OF AIR.*'*

§ 1. CONTINUOUS observation of natural atmospheric electricity has given ample proof that cloudless air at moderate heights above the earth's surface, in all weathers, is electrified with very far from homogeneous distribution of electric density. Observing, at many times from May till September, 1859, with my portable electrometer on a flat open sea-beach of Brodick Bay in the Island of Arran, in ordinary fair weather at all hours of the day, I found the difference of potentials, between the earth and an insulated burning match at a height of 9 feet above it (2 feet from the uninsulated metal case of the instrument, held over the head of the observer), to vary from 200 to 400 Daniell's elements, or as we may now say volts, and often during light breezes from the east and northeast it went up to 3,000 or 4,000 volts. In that place, and in fair weather, I never found the potential other than positive (never negative, never even down to zero), if for brevity we call the earth's potential at the place zero. In perfectly clear weather under a sky sometimes cloudless, more generally somewhat clouded, I often observed the potential at the 9 feet height to vary from about 300 volts gradually to three or four times that amount, and gradually back again to nearly the same lower value in the course of about two minutes.* I inferred that these gradual variations must have been produced by

* Two communications by Lord Kelvin, P.R.S., to the Philosophical Society of Glasgow, meeting in the Natural Philosophy lecture-room of the University of Glasgow, March 27, 'On the Electrification of Air'; 'On the Thermal Conductivity of Rock at Different Temperatures.' Printed from proof sheets for *Nature* contributed by the author.

* 'Electrostatics and Magnetism,' Sir William Thomson. xvi. §§ 281, 282.

electrified masses of air moving past the place of observation. I did not remark then, but I now see, that the electricity in these moving masses of air must, in all probability, have been chiefly positive to cause the variations which I observed, as I shall explain to you a little later.

§ 2. Soon after that time a recording atmospheric electrometer * which I devised, to show by a photographic curve the continuous variation of electric potential at a fixed point, was established at the Kew Meteorological Observatory, and has been kept in regular action from the commencement of the year 1861 till the present time. It showed incessant variations quite of the same character, though not often as large, as those which I had observed on the sea-beach of Arran.

Through the kindness of the Astronomer Royal, I am able to place before you this evening the photographic curves for the year 1893, produced by a similar recording electrometer which has been in action for many years at the Royal Observatory, Geeenwich. They show, as you see, not infrequently, during several hours of the day or night, negative potential and rapid transitions from large positive to large negative. Those were certainly times of broken weather, with at least showers of rain, or snow, or hail. But throughout a very large proportion of the whole time the curve quite answers to the description of what I observed on the Arran sea-beach thirty-six years ago, except that the variations which it shows are not often of so large amount in proportion to the mean or to the minimums.

§ 3. Thinking over the subject now, we see that the gradual variations, minute after minute through so wide a range as the 3 or 4 to 1, which I frequently observed, and not infrequently rising to twenty times the ordinary minimum, must have been due to *positively* electrified masses of air, within a few hundred feet of the place of observation, wafted along with the gentle winds of 5 or 10 or 15 feet per second which were blowing at the time. If any comparably large quantities of negatively electrified air had been similarly carried past, it is quite certain that the minimum observed potential, instead of being in every case positive, would have been frequently large negative.

§ 4. Two fundamental questions in respect to the atmospheric electricity of fair weather force themselves on our attention:—(1) What is the cause of the prevalent positive potential in the air near the earth, the earth's potential being called zero? (2) How comes the lower air to be electrified to different electric densities whether positive or negative in different parts? Observations and laboratory experiments made within the last six or eight years, and particularly two remarkable discoveries made by Lenard, which I am going to describe to you, have contributed largely to answering the second of these questions.

§ 5. In an article 'On the Electrification of Air by a Water-jet,' by Magnus Maclean and Makita Goto,* experiments were described showing air to be negatively electrified by a jet of water shot vertically down through it from a fine nozzle into a basin of water about 60 centimeters below it. It seemed natural to suppose that the observed electrification was produced by the rush of the fine drops through the air; but Lenard conclusively proved, by elaborate and searching experiments, that it was in reality due chiefly, if not wholly, to the violent commotions of the drops impinging on the water surface of the receiving basin, and he found that the negative electrification of the air was greater when they were allowed to fall on a hard slab of any material thoroughly wetted by water than when they fell on a yielding surface of water several

* 'Electrostatics and Magnetism.'

* *Philosophical Magazine*, 1890, second half-year.

centimeters deep. He had been engaged in studying the great negative potential which had been found in air in the neighborhood of waterfalls, and which had generally been attributed to the inductive action of the ordinary fine weather electric force, giving negative electricity to each drop of water-spray before it breaks away from conducting communication with the earth. Before he knew Maclean and Goto's paper, he had found strong reason for believing that that theory was not correct, and that the true explanation of the electrification of the air must be found in some physical action not hitherto discovered. A less thorough inquirer might have been satisfied with the simple explanation of the electricity of waterfalls naturally suggested by Maclean and Goto's result, and might have rested in the belief that it was due to an electrifying effect produced by the rush of the broken water through the air; but Lenard made an independent experimental investigation in the Physical Laboratories of Heidelberg and Bonn, by which he learned that the seat of the negative electrification of the air electrified is the lacerated water at the foot of the fall, or at any rocks against which the water impinges, and not the multitudinous interfaces between air and water falling freely in in drops through it.

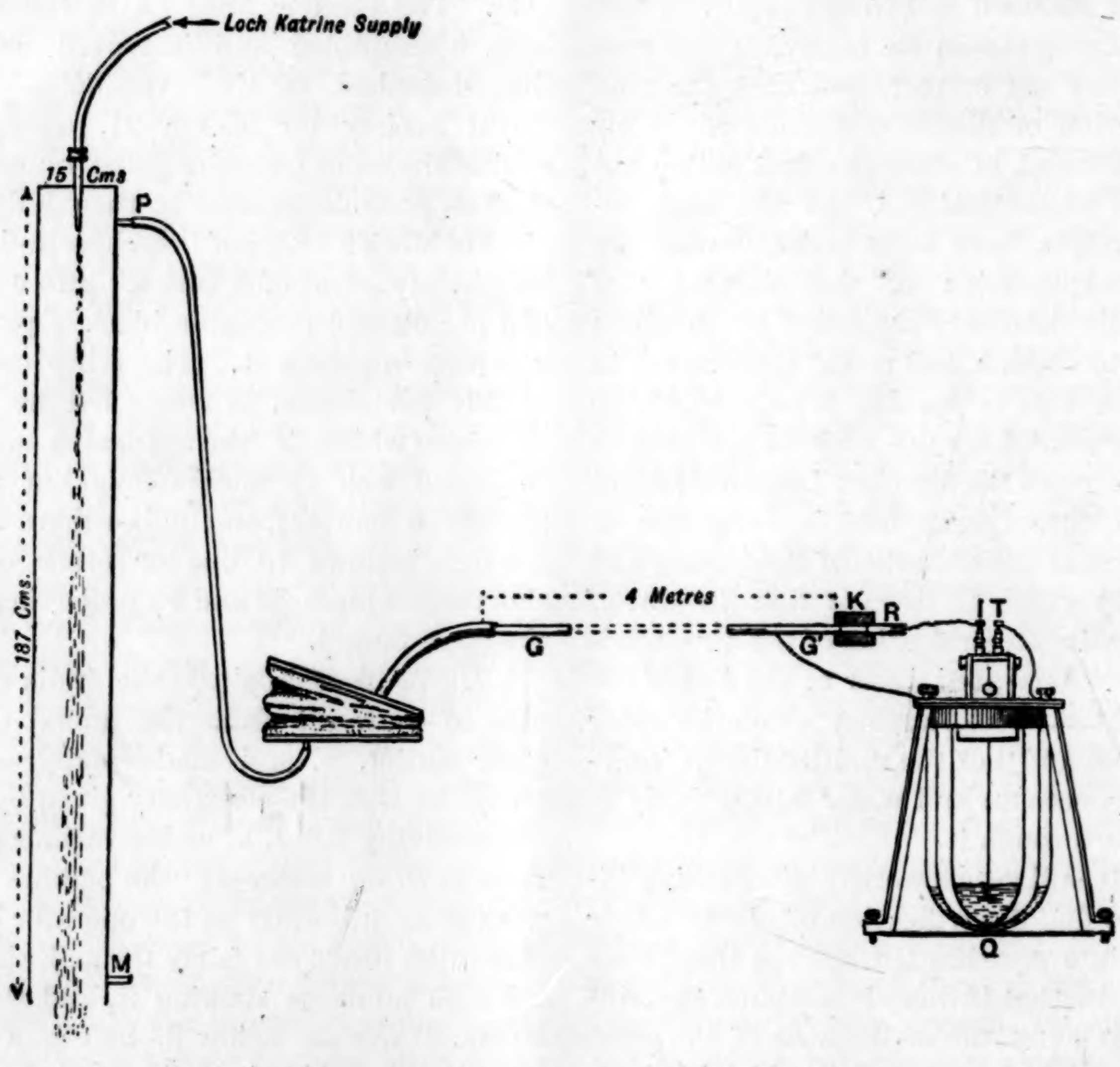
§ 6. It still seems worthy of searching inquiry to find electrification of air by water falling in drops through it, even though we now know that if there is any such electrification it is not the main cause of the great negative electrification of air which has been found in the neighborhood of waterfalls. For this purpose an experiment has been very recently made by Mr. Maclean, Mr. Galt and myself, in the course of an investigation regarding electrification and diselectrification of air with which we have been occupied for more than a year. The apparatus which we used is before you. It consists of a quadrant electrometer connected with an insulated electric filter* applied to test the electrification of air drawn from different parts of a tinned iron funnel, 187 centimeters long and 15 centimeters diameter, fixed in a vertical position with its lower end open and its upper end closed, except a glass nozzle, of 1.6 mm. aperture admitting a jet of Glasgow supply water (from Loch Katrine) shot vertically down along its axis. The electric filter (R in the drawing), a simplified and improved form of that described in the *Proceedings* of the Royal Society for March 21, consists of twelve circles of fine wire gauze rammed as close as possible together in the middle of a piece of block tin pipe of 1 cm. bore and 2 cm. length. One end of it is stuck into one end of a perforation through a block of paraffin, K, which supports it. The other end (G) of this perforation is connected by block tin pipe (which in the apparatus actually employed was 4¾ meters long, but might have been shorter), and india-rubber tubing through bellows to one or other of two short outlet pipes (M and P) projecting from the large funnel.

§ 7. We first applied the india-rubber pipe to draw air from the funnel at the *upper* outlet, P, and made many experiments to test the electricity given by it to the receiving filter, R, under various conditions as to the water-jet; the bellows being worked as uniformly as the operator could. When the water fell fairly through the funnel with no drops striking it, and through 90 cm. of free air below its mouth, a small negative electrification of R was in every case observed (which we thought might possibly be attributable to electrification of air where the water was caught in a basin about 90 cm. below the mouth of the funnel). But when the funnel was slanted so that the whole shower of drops from the jet, or even a small part of it, struck

* Kelvin, Maclean, Galt, 'On the Diselectrification of Air.' *Proc.* Roy. Soc., March 14, 1895.

the inside of the funnel the negative electrification of R was largely increased. So it was also when the shower, after falling freely down the middle of the funnel, impinged on a metal plate in metallic communication with the funnel, held close under its mouth, or 10 or 20 cm. below it. For example, in a series of experiments made volt in five minutes with no obstruction; and 6.78 volts in five minutes with the metal plate held below the mouth as before.

§ 8. These results, and others which we have found, with many variations of detail, confirm, by direct test of air drawn away from the neighborhood of the waterfall through a narrow pipe to a distant



last Monday (March 25), we found .28 of a volt in 15 minutes with no obstruction to the shower; and 4.18 volts in five minutes, with a metal plate held three or four centimeters below the mouth of the funnel; the air being drawn from the upper outlet (P). Immediately after, with P closed, and air drawn from the lower outlet (M), but all other circumstances the same, we found .20 of a electrometer, Lenard's conclusion that a preponderatingly strong negative electrification is given to the air at every place of violent impact of a drop against a water-surface or against a wet solid. But they do not prove that there is *no* electrification of air by drops of water falling through it. We always found, in every trial, decisive proof of negative electrification; though of

comparatively small amount when there was no obstruction to the shower between the mouth of the funnel and the catching basin 90 cm. below it. We intend to continue the investigation, with the shower falling freely far enough down from the mouth of the funnel to make quite sure that the air which we draw off from any part of the funnel is not sensibly affected by impact of the drops on anything below.

§ 9. The other discovery * of Lenard, of which I told you, is that the negative electrification of air, in his experiments with pure water, is diminished greatly by very small quantities of common salt dissolved in it, that is brought to nothing by .011 per cent.; that positive electrification is produced in the air when there is more than .011 per cent. of salt in the water, reaching a maximum with about 5 per cent. of salt, when the positive electrical effect is about equal to the negative effect observed with pure water, and falling to 14 per cent. of this amount when there is 25 per cent. of salt in the solution. Hence sea-water, containing as it does, about 3 per cent. of common salt, may be expected to give almost as strong positive electrification to air as pure water would give of negative in similar circumstances as to commotion. Lenard infers that breaking waves of the sea must give positive electricity to the air over them; he finds, in fact, a recorded observation by Exner, on the coast of Ceylon, showing the normal positive electric potential of the air to be notably increased by a storm at sea. I believe Lenard's discovery fully explains also some very interesting observations of atmospheric electricity of my own, which I described in a letter to Dr. Joule, which he published in the *Proceedings* of the Literary and Philosophical Society of Manchester for October 18, 1859. * "The atmospheric effect ranged from 30° to about 420° [of a heterostatic torsion electrometer of 'the divided-ring' species] during the four days which I had to test it; that is to say, the electrometric force per foot of air, measured horizontally from the side of the house, was from 9 to above 126 zinc-copper water cells. The weather was almost perfectly settled, either calm, or with slight east wind, and in general an easterly haze in the air. The electrometer twice within half an hour went above 420°, there being at the time a fresh temporary breeze from the east. What I had previously observed regarding the effect of east wind was amply confirmed. Invariably the electrometer showed very high positive in fine weather, before and during east wind. It generally rose very much shortly before a slight puff of wind from that quarter, and continued high till the breeze would begin to abate. I never once observed the electrometer going up unusually high during fair weather without east wind following immediately. One evening in August I did not perceive the east wind at all, when warned by the electrometer to expect it; but I took the precaution of bringing my boat up to a safe part of the beach, and immediately found by waves coming in that the wind must be blowing a short distance out at sea, although it did not get so far as the shore . . . . On two different mornings the ratio of the house to a station about sixty yards distant on the road beside the sea was .97 and ·96 respectively. On the afternoon of the 11th inst, during a fresh temporary breeze of east wind, blowing up a little spray as far as the road station, most of which would fall short of the house, the ratio was 1.08 in favour of the house electrometer—both standing at the time very

* 'Ueber die Electricität der Wasserfälle.' Table xvii. p. 228. *Annalen der Physik und Chemie*, 1892, vol. xlvi.

* Republished in 'Electrostatics and Magnetism.' 'Atmospheric Electricity,' xvi. § 262.

high—the house about 350°. I have little doubt but that this was owing to the negative electricity carried by the spray from the sea, which would diminish relatively the indications of the road electrometer."

§ 10. The negative electricity spoken of in this last sentence, 'as carried by the spray from the sea,' was certainly due to the inductive effect of the ordinary electrostatic force in the air close above the water, by which every drop or splash breaking away from the surface must become negatively electrified; but this only partially explains the difference which I observed between the road station and the house station. We now know, by the second of Lenard's two discoveries, to which I have alluded, that every drop of the salt water spray, falling on the ground or rocks wetted by it, must have given positive electricity to the adjoining air. The air, thus positively electrified, was carried towards and over the house by the on-shore east wind which was blowing. Thus, while the road electrometer under the spray showed less electrostatic force than would have been found in the air over it and above the spray, the house electrometer showed greater electrostatic force because of the positively electrified air blowing over the house from the wet ground struck by the spray.

§ 11. The strong positive electricity, which as described in my letter to Joule, I always found in Arran with east wind, seemed at first to be an attribute of wind from that quarter. But I soon found that in other localities east wind did not give any very notable augmentation, nor perhaps any augmentation at all, of the ordinary fair weather positive electric force, and for a long time I have had the impression that what I observed in this respect, on the sea-beach of Brodick Bay in Arran, was really due to the twelve nautical miles of sea between it and the Ayrshire coast, east-north-east of it; and now it seems to me more probable than ever that this is the explanation when we know from Lenard that the countless breaking waves, such as even a gentle east wind produces over the sea between Ardrossan and Brodick, must every one of them give some positive electricity to the air wherever a spherule of spray falls upon unbroken water. It becomes now a more and more interesting subject for observation (which I hope may be taken up by naturalists having the opportunity) to find whether or not the ordinary fine weather positive electric force at the sea coast in various localities is increased by gentle or by strong winds from the sea, whether north, south, east or west of the land.

§ 12. From Lenard's investigation we now know that every drop of rain falling on the ground or on the sea,* and every drop of fresh water spray of a breaking wave, falling on a fresh water lake, sends negative electricity from the water surface to the air; and we know that every drop of salt water, falling on the sea from breaking waves, sends positive electricity into the air from the water surface. Lenard remarks that more than two-thirds of the earth's surface is sea, and suggests that breaking sea-waves may give contributions of positive electricity to the air which may possibly preponderate over the negative electricity given to it from other sources, and may thus be the determining cause of the normal fair weather positive of natural atmospheric electricity. It seems to me highly probable that this preponderance is real for atmospheric electricity at sea. In average weather, all the year round, sailors in very small vessels are more wet by sea-spray than by rain, and I think it almost certain that more positive electricity is given to the air by breaking waves than negative elec-

* 'Ueber die Electricität der Wasserfälle.' *Annalen der Physik und Chemie*, 1892, vol. xlvi., p. 631.

tricity by rain. It seems also probable that the positive electricity from the waves is much more carried up by strong winds to considerable heights above the sea than the negative electricity given to the air by rain falling on the sea; the greater part of which may be quickly lost into the sea, and but a small part carried up to great heights. But it seems to me almost certain that the exceedingly rapid recovery of the normal fair weather positive, after the smaller positive or the negative atmospheric electricity of broken weather, which was first found by Beccaria in Italy 120 years ago, and which has been amply verified in Scotland and England, *could not be accounted for by positively electrified air coming from the sea. Even at Beccaria's Observatory, at Garzegna di Mondovi in Piedmont, or at Kew or Greenwich or Glasgow, we should often have to wait a very long time for reinstatement of the normal positive after broken weather, if it could only come in virtue of positively electrified air blowing over the place from the sea; and several days, at least, would have to pass before this result could possibly be obtained in the centre of Europe.

§ 13. It has indeed always seemed to me probable that the rain itself is the real restorer of the normal fair weather positive. Rain or snow, condensing out of the air high up in the clouds, must itself, I believe, become positively electrified as it grows, and must leave positive electricity in the air from which it falls. Thus rain falling from negatively electrified air would leave it less negatively electrified, or non-electrified or positively electrified; rain falling from non-electrified air would leave it positively electrified; and rain falling from positively electrified air would leave it with more of positive electricity than it had before it lost water from its composition. Several times within the last thirty years I have made imperfect and unsuccessful attempts to verify this hypothesis by laboratory experiments, and it still remains unproved. But I am much interested just now to find some degree of observational confirmation of it in Elster and Geitel's large and careful investigation of the electricity produced in an insulated basin by rain or snow falling into it, which they described in a communication published in the *Sitzungsberichte* of the Vienna Academy of Sciences, of May, 1890. They find generally a large electrical effect, whether positive or negative, by rain or snow falling into the basin for even so short a time as a quarter of a minute, with however, on a whole, a preponderance of negative electrification.

§ 14. But my subject this evening is not merely natural atmospheric electricity, although this is certainly by far the most interesting to mankind of all hitherto known effects of the electrification of air. I shall conclude by telling you very briefly, and without detail, something of new experimental results regarding electrification and diselectrification of air, found within the last few months in our laboratory here by Mr. Maclean, Mr. Galt and myself. We hope before the end of the present session of the Royal Society to be able to communicate a sufficiently full account of our work.

§ 15. Air blown from an uninsulated tube, so as to rise in bubbles through pure water in an uninsulated vessel, and carried through an insulated pipe to the electric receiving filter, of which I have already told you, gives negative electricity to the filter. With a small quantity of salt dissolved in the water, or sea water substituted for fresh water, it gives positive electricity to the air. There can be no doubt but these results are due to the same physical cause as Lenard's negative and positive electrification of air by the impact of drops of fresh water or of salt water on a surface of water or wet solid.

* 'Electrostatics and Magneticism,' XVI., § 287.

§ 16. A small quantity of fresh water or salt water shaken up vehemently with air in a corked bottle electrifies the air, fresh water negatively, salt water positively. A 'Winchester quart' bottle (of which the cubic contents is about two litres and a half), with one-fourth of a litre of fresh or salt water poured into it, and closed by an india-rubber cork, serves very well for the experiment. After shaking it vehemently till the whole water is filled with fine bubbles of air, we leave it till all the bubbles have risen and the liquid is at rest, then take out the cork, put in a metal or india-rubber pipe, and by double-acting bellows draw off the air and send it through the electric filter. We find the electric effect, negative or positive, according as the water is fresh or salt, shown very decidedly by the quadrant electrometer; and this, even if we have kept the bottle corked for two or three minutes after the liquid has come to rest before we take out the cork and draw off the air.

§ 17. An insulated spirit lamp or hydrogen lamp being connected with the positive or with the negative terminal of a little Voss electric machine, its fumes (products of combustion mixed with air) sent through a block-tin pipe, four meters long, and one centimeter bore, ending with a short insulating tunnel of paraffin and the electric filter, gives strong positive or strong negative electricity to the filter.

§ 18. Using the little biscuit-canister and electrified needle, as described in our 'communication'* to the Royal Society 'On the Diselectrification of Air,' but altered to have two insulated needles with varied distances of from a half a centimeter to two or three centimeters between them, we find that when the two needles are kept at equal differences of potential positive and negative, from the enclosing metal canister, little or no electrification is shown by the electric filter; and when the differences of potential from the surrounding metal are unequal, electrification, of the same sign as that of the needle whose difference of potential is the greater, is found on the filter.

When a ball and needle-point are used, the effect found depends chiefly on the difference of potentials between the needle-point and the surrounding canister, and is comparatively little affected by opposite electrification of the ball. When two balls are used, and sparks in abundance pass between them, but little electricity is deposited by the sparks in the air, even when one of the balls is kept at the same potential as the surrounding metal. [The communication was illustrated by a repetition of some of the experiments shown on the occasion of a Friday evening lecture* on Atmospheric Electricity at the Royal Institution on May 18, 1860, in which one-half of the air of the lecture-room was electrified positively, and the other half negatively, by two insulated spirit lamps mounted on the positive and negative conductors of an electric machine.]

* *Proceedings* of the Royal Society, March 14, 1895.

(2) '*ON THE THERMAL CONDUCTIVITY OF ROCK AT DIFFERENT TEMPERATURES.*'

EXPERIMENTS by Lord Kelvin and Mr. Erskine Murray were described, and the apparatus used in them was shown, by which it was found that the thermal conductivity of specimens of slate, sandstone and granite is less at higher temperatures than at lower for each of these rocks. The last tested was Aberdeen granite, for which experiments of fairly satisfactory accuracy showed the mean conductivity for the range from 146° C. to 215° C. to be 86 per cent. of the mean conductivity in the range from 81° C. to 146° C. They hope to send a communication to the Royal Society describing their work before the end of the present session.

* 'Electrostatics and Magnetism,' xvi., §§ 285, 286.

A DYNAMICAL HYPOTHESIS OF INHERITANCE.* †

THE doctrine of the preformation of an organism in the germ is as inconsistent with the fact as with the requirements of dynamical theory. The effects of the preconceptions of preformationism have been only too apparent in framing hypotheses of inheritance. The now dominant hypothesis is simply an amplification, in the light of numerous modern facts, of the preformationism of Democritus. He supposed that almost infinitesimally small and very numerous bodies were brought together in the germ from all parts of the body of the parent. These minute representative corpuscles were supposed to have power to grow, or germinate, at the right time, and in the right order, into the forms of the parts and organs of the new being. In this way it was supposed that the characteristics of the parent were represented in a latent form in the germ, which might grow as a whole, by the simultaneous and successive development of the germinal aggregate composed, so to speak, of excessively minute buds, or rudiments of the organs. In such wise also did the successors of Democritus, namely, Aristotle, Buffon, and Erasmus Darwin, suppose that the inheritance of parental likeness by offspring was to be explained. The later and greater Darwin greatly amplified this hypothesis and proposed, provisionally, to account for the phenomena of inheritance by its help. Conceiving the process somewhat as above supposed, he consistently gave to his provisional hypothesis the name of *pangenesis*, since the minute latent buds of the germ were supposed to come from, and thus represent potentially every part of the bodies of the parents, and possibly of still remoter ancestry.

With the discovery of the presence of germinal substance in multicellular organisms, from the embryonic stages onwards, by Owen, Galton, Jäger, Nussbaum and others, the theory of continuity of germinal matter came into vogue. Upon this basis Weismann distinguished two kinds of plasma in multicellular beings, namely, the germ-plasm and the body-plasm, and at first assumed that because of this separation the latter could not modify the former, since the fate of the respective sorts of plasma was predetermined by virtue of this separation. The one kind was the mere carrier of the other, and the germ-plasm was immortal because it was produced in each species from a store of it which always existed, either in a latent or palpable form, from the very beginning of development. He seems, however, in recent years, to have admitted that this germ-plasm could be indirectly modified in constitution through the influence of the body-plasm that bore and enclosed it. Beyond this point Weismann again becomes a preformationist, as truly as Democritus, in that he now conjectures that the supposed innumerable latent buds of the germ, representative of the organs of the future being, are minute masses which he sees as objective realities in the chromosomes of the nuclei of the sex-cells. These chromosomes of the germ he calls 'ids' and 'idants,' according to their condition of sub-division, and supposes them to grow and become divided into 'determinants' and 'biophors' in the course of embryonic development. To these he ascribes powers little short of miraculous, in that he asserts that these infinitesimal germinal particles grow and divide just at the right time and order, and control de-

*From 'The Biological Lectures' of the Marine Biological Laboratory, Vol. III., 1895. Printed from the proofs by the courtesy of the editor, Professor Whitman.

† It is interesting to note that the views developed in this lecture lead to conclusions in some respects similar to those held by Professor Whitman in his discourse entitled: 'The Insufficiency of the Cell-Theory of Development,' published in the series of lectures delivered in 1893.

velopment so as to build up anew the arrangement of parts seen in the parent type. This elaborate system of preformationism is bound to produce a reaction that is already becoming apparent; in fact, it is probable that its very complexity, its many inconsistencies, as well as the numerous subsidiary hypotheses that must be worked out to support it, will be fatal to it as a system.

The path along which the solution of the problem of heredity is to be effected lies in a wholly different direction, namely, in that of the study of the mechanics and dynamics of development, and in the resolute refusal to acknowledge the existence of anything in the nature of preformed organs or of infinitesimal gemmules of any kind whatsoever. Such devices are unnecessary and a hindrance to real progress in the solution of the questions of inheritance. They only serve to divert the attention of the observer from the real phenomena in their totality to a series of subordinate details, as has happened in Weismann's case. All this while he has been watching the results of an epigenetic process, as displayed by an inconceivably complex mechanism in continuous transformation, and out of all of this the most essential thing he has witnessed has been one of the *effects* of the operation of that contrivance in the mere splitting of chromosomes that are his 'ids,' 'idants,' 'biophors,' etc. The potentiality of the part has been mistaken for that of the whole.

We must dismiss from our minds all imaginary corpuscles as bearers of hereditary powers, except the actual chemical metameric or polymeric molecules of living matter, as built up into ultramicroscopic structures, if we wish to frame an hypothesis of heredity that is in accord with the requirements of dynamical theory. The 'discovering' and naming of 'ids,' 'biophors' and 'pangenes' time will show to have been about as profitable as sorting snowflakes with a hot spoon. We must also dismiss the idea that the powers of development are concentrated in some particular part of the germ-cell, nor can we assume the latter to be homogenous.* This we are compelled to deny on the ground of the organization of the egg itself. Nor is it possible to deny the reciprocal effects of cells upon each other; the parts are reciprocals of the whole, as the latter is reciprocal to a part. The organism during every phase of its existence is a molecular mechanism of inconceivable complexity, the sole motive force of which is the energy that may be set free by the coördinated transformation of some of its molecules by metabolism. An appeal to anything beyond this and the successive configurations of the molecular system of the germ, as a whole, resulting from the changing dynamical properties of its molecules, as their individual configurations and arrangement change, must end in disappointment. We must either accept such a conclusion or deny that the principle of the conservation of force holds in respect to the behavior of the ultimate molecular constituents of living substance. But to deny that that principle is operative in living creatures is to question direct experimental evidence to the contrary, since Rübner has been able to actually use an organism as a fairly accurate calorimeter.

The initial configuration or mechanical arrangement and successive rearrangements of the molecules of a germ, the addition of

*The writer finds himself unable to agree with Haacke, if he has properly understood that author's assumption as to the homogeneity or monotonous character of living matter, as set forth in his admirable work *Gestaltung und Vererbung*, 1893. Nor does it appear that anything is gained by the acceptance of Haacke's theory of Gemmaria that is not easily understood upon the far simpler grounds that will be set forth here, though there is much in the book cited with which epigenesists must agree, aside from the weighty character of its criticisms and its pregnant suggestiveness.

new ones by means of growth, plus their chemical and formal transformation as an architecturally self-adjusted aggregate, by means of metabolism, is all that is required in an hypothesis of inheritance. The other properties of living matter, such as its viscosity, free and interfacial surface-tension, osmotic properties, its limit of saturation with water, its segmentation into cells, in short, its organization, must be the result of the operation of forces liberated by its own substance during its growth by means of metabolism. We cannot exclude external forces and influences, such as chemism, light, heat, electricity, gravity, adhesion, exosmosis, food, water, air, motion, etc., in the operation of such a complex mechanism. It is these agencies that are the operators of the living mechanism, which in its turn makes certain successive responses in a way that is determined within limits by its own antecedent physical structure and consequent dynamical properties. The parts of the whole apparatus are kept in a condition of continuous 'moving equilibrium' by external agencies, to borrow a phrase of Mr. Spencer's.

This view, it will be seen, leads to a determinism as absolute as that of the Neo-Darwinists, but upon a wholly different basis. It leads to the denial of the direct mutability of the germ by any means other than the transformation, chemical and structural, through metabolism of the germinal mechanism. It not only compels us to deny that the germ can be at once so effected by external blows as to transmit changes thus produced hereditarily except under exceptional conditions, as we shall see later. It denies also, by implication, that the cytoplasm can be so modified, except indirectly, or through architectural transformations of its ultramicroscopic structure.

It is also compelled to deny that spontaneous or autogenous characters can either arise or be transmitted without involving the principle of the conservation or correlation of force, since no transformation of such a mechanism can take place without involving forces directly or indirectly exerted by the external world. In short, the energy displayed by a living molecular system from within must be affected by energies coming upon it from without. All characters whatsoever were so acquired, so that the truth is that there are no others to be considered. Characters acquired through the interaction of inner and outer forces are the only ones possible of acquirement.

That through reciprocal integration (fertilization and formation of an oösperm) this rule may have apparent exceptions, through the compounding of two molecular mechanisms of different strengths, dynamically considered, it is impossible to deny in the face of the evidence of breeders. Such exceptions are apparent, however, and not real, as must follow from dynamical theory.

The sorting process, called natural selection, is itself dynamic, and simply expresses the fact that, by an actual operation with a living body of a certain kind, something more than a balancing of forces is involved between internal and external energies whenever a survival occurs. The principles of dynamics therefore apply in all strictness to natural selection.

What it is that makes crosses or hybrids more variable and often more vigorous than inbred forms must also have a dynamic explanation, since there can be no increased activity of metabolic processes without an increased expenditure of energy and an increased rate of molecular transformation.

Variations cannot be spontaneous, as Darwin himself was aware. The only way in which they can be supposed to have arisen is by the blending of molecular dynamical systems of differing initial potential strengths, by the conjugation of sex-cells (reciprocal integration), and by means of variations in the interactions of such result-

ant systems with their surroundings. This, however, Weismann and his followers deny, though no proof whatever has been offered that such is not the fact. Indeed, it is probable that, so long as the ultimate machinery of metabolism is beyond the reach of ocular demonstration, there can be no proof or disproof of the position assumed by the performationists or Neo-Darwinists. Such proof or disproof is, however, non-essential, since we are forbidden by the first principles of dynamics to assume that transformation of any living physical system whatever can occur without involving some forces or influences that emanate from the external world.* The separation and evaluation of the internal and external forces incident to the manifestation of life, in the present state of our knowledge, and from the very nature of the case, plainly transcends the capacity of present available experimental methods in biology. The discussion as to whether 'acquired characters' are inherited can, therefore, have but one outcome, since external forces can never be excluded in considering the life-history of any organism.

Nägeli, in seeking to account for the phenomena of growth, gave us a most ingenious physical hypothesis of the constitution of living matter. This, later on, he modified so as to develop an hypothesis of hereditary transmission. But the micellæ that were representative of the germinal matter of a species he isolated in the form of rows or chains of micellæ traversing the rest of the living substance of the organism, and called it *idioplasm*. Here again the germinal matter was conceived as separate from that forming the rest of the body. Mr. Spencer supposed "that sperm-cells and germ-cells are essentially nothing more than vehicles in which are contained small groups of the physiological units in a fit state for obeying their proclivity towards the structural arrangement of the species they belong to." These 'physiological units' are neither chemical nor morphological in character, according to Mr. Spencer's system, but it is admitted that their properties and powers must be determined in some way by their own constitution, conditions of aggregation, and relation to the outer world. The views of Nägeli and Spencer are akin in certain respects, but they still retain a certain amount of resemblance to the older ones, namely, those hypotheses which assume that the forces of inheritance are lodged in certain very small corpuscles forming part only of the germ or organism. These hypotheses are also dynamical in nature, and have been worked out with the consciousness, in both cases, that the mechanism of inheritance must also be the one through which metabolism operates. Indeed, these two authors seem to be the first to have distinctly recognized the necessity for such a supposition.

Later still, with the advent of the discovery that the male nucleus was fused with the female nucleus during sexual reproduction, it was assumed that the nuclear contents were the only essential material bearers of those hereditary forces that shape the growing germ into the likeness of the parentage. With the development of this idea the name of Weismann is perhaps most closely associated. He has utilized the facts of development, nuclear cleavage, expulsion of polar bodies, halving and subdivision of chromosomes, etc., as the founda-

*"Some of the exponents of this [preformation] theory of heredity have attempted to elude the difficulty of placing a whole world of wonders within a body so small and so devoid of structure as a germ, by using the phrase structureless germs (F. Galton, Blood-relationship, *Proc. Roy. Soc.*, 1872). Now one material system can differ from another only in the configuration and motion which it has at a given instant. To explain differences of function and development of a germ without assuming differences of structure is, therefore, to admit that the properties of a germ are not those of a purely material system."—JAMES CLERK-MAXWELL, article Atom, *Encycl. Britan.*, 9th ed., Vol. III., p. 42, 1878.

tion of his hypothesis of inheritance. Its extreme elaboration is its greatest weakness, and in it, no less than in all preceding hypotheses, the theory of a separate category of particles carrying hereditary potentialities again appears.

The one criticism that holds of all these hypotheses is that they are one-sided and ignore a most important set of factors in inheritance, namely, the purely statical ones, or those arising from the mere physical properties of the living matter of the germ viewed as if it were a dead, inert mass, subject to the operation of the reciprocal attraction for one another of its constituent particles. All of these hypotheses, moreover, assume that it is only *some* of the matter of the germ that is concerned in the process of hereditary transmission, and that the remainder may be regarded as passive. The entire germ, on the contrary, or all of it that undergoes development, must be considered as a single whole, made up of a vast number of molecules built up into a mechanism. Such a molecular mechanism, it must be supposed, cannot set free the potential energy of its parts except in a certain determinate order and way, within certain limits, in virtue of the initial physical structure of the whole. If the germ is free to do that, as must happen under proper conditions, as a mechanism, its parts, as they are thus formed by their own metabolism, it may be assumed, will inevitably and nearly recapitulate the ancestral development or that typical of the species. It must do this as a mere dynamical system or mechanism, the condition of which at one phase determines that of the next, and so on, to the completion of development.

In the present state of our knowledge we are not prepared to frame a purely mechanical hypothesis of inheritance that shall answer every requirement, in spite of the fact that no other is possible. Herbert Spencer and Professor Haeckel long ago pointed out that such an hypothesis is a necessity growing out of the very requirements that must be satisfied in any attempt to coördinate the phenomena of biology with those of the not-living world. The material basis of life is always a chemically and mechanically compounded substance. To the very last molecule, such a body must betray evidence of arrangement or structure of its parts that should make it a mechanism of the utmost complexity and requisite potentiality as a transformer of energy through the mere transposition and rearrangement of such parts. We find indeed that living matter is chemically the most complex and unstable substance known. It is composed largely of carbon, a quadrivalent element that stands alone in its power to combine with itself and at the same time hold in chemical bondage groups of atoms representing other chemical bodies. Such groups are probably held together in great numbers metamerically by the reciprocal or otherwise unsatisfied affinities of the large number of carbon atoms entering into the composition of the proteid molecule. In this way the massive and structurally complex molecule of protoplasm may be supposed to have arisen. We may thus trace the genesis of the peculiarities of living matter to this singular property of the carbon atom. On such a basis we may suppose that the ultimate molecular units are identical with the physiological units, so that their structures may not only determine the nature of the metabolism they can undergo, but also be the ultimate units of form or morphological character.

What especially gives color to these suspicions is the extraordinary variety of changes, alteration of properties or powers, and the vast variety of living matter, as represented by the million or more of known distinct living species of organisms. It is as if the permutations, transformations, and the dynamical readjustment of the meta-

meres of the molecules of living matter were the source of its varying potentialities as manifested in its protean changes of specific form and function. That some mechanical and consequently dynamical interpretation of these transformations may yet be forthcoming is, I take it, distinctly foreshadowed by the advances in the newer theories of stereo-chemistry developed by LeBel and Van't Hoff. If this is the case we may yet hope for a mechanical and dynamical explanation of the phenomena of life and inheritance. Especially is this true if we further suppose that the large molecules of living plasma are rather feebly held together by a force almost of the nature of cohesion. We may be permitted thus to find an explanation of that phenomenon which is always so characteristic of living matter, namely, the large and relatively fixed amount of water it contains, and also the mobility of its molecules in respect to one another, its jelly-like character at one instant, its fluidity and power of motion at another. It is indeed probable that the amount of water contained in living matter is controlled within certain limits by the forces of cohesion exerted between adjacent molecules against the osmotic pressure or capillary action of water tending to drive them asunder, as supposed by Nägeli, in his hypothesis of micellæ. Such an hypothesis enables us to explain much that is otherwise quite unintelligible in relation to living things. It renders us an explanation of amœboid motion, of the surface tensions of protoplasm and lastly of metabolism itself through osmosis and the specific characters of the chemical transformations that must take place in each kind of living substance.

Such an hypothesis may also afford us mechanical constructions of atoms, grouped into very large metameric or polymeric molecules of the utmost diversity of powers, capable of undergoing a long series of successive transformations, so as to manifest in the long run, starting with a molecular germinal aggregate, what we call ontogeny or development. These transformations, we must suppose, are effected by the metabolism incident to growth, and moreover, that starting with an initial configuration of a system of molecules, as a mechanical and consequently a dynamical system of determinate powers, in the form of a germ, it cannot undergo any other transformations except such as lead to an approximate recapitulation of the ancestral development or phylogeny. This supposition follows from the rule that must hold of determinate systems of molecules, as well as of systems formed of larger masses, namely, that the initial changes in the configuration of such a complex system must dynamically determine within certain variable limits, under changing conditions, the nature of all of its subsequent transformations, including those due to growth and consequently increased complexity. We thus escape the necessity of invoking certain 'proclivities' of physiological units, or the necessity of appealing to the growth and fission of 'biophors' or the scattering of 'determinants' at the proper times and places in the course of development. We thus escape, too, the mistake of assuming that a part of a germ controls the whole, a proposition that has been so long advocated by one school of biologists that it is astounding that its fallacy has not long since been more generally understood. Such a doctrine is not credible in the face of the fact that we know of no development except that which takes place in intimate association with cytoplasm, which seems to be the principal theater of metabolism and growth. We cannot conceive of the transformations of a germ without considering the metabolism of all its parts, such as nucleus, cytoplasm, centrosomes, archoplasm, chromatin, spindles, astral figures, microsomata, etc.

'Tendencies' and 'proclivities' are words that have no legitimate place in the discussion of the data of biology any more than they have in natural philosophy or physics. Karyokinesis, now admittedly inseparable in thought from the idea of multicellular development, is a rhythmical process so complex in its dynamical aspects as to some extent lead one unwittingly to underestimate the absolute continuity of the accompanying processes of metabolism. But that is no reason why the importance of nuclear metamorphosis should be exaggerated at the expense of the far more important forces developed by metabolism and growth. In fact, the 'ids,' 'idants,' etc., of that school of biologists are not causes but mere effects, produced as passing shadows, so to speak, in the operation of the perfectly continuous processes of metabolism incident to development. Reciprocal relations are sustained between nucleus and cytoplasm of such importance that the transformation or fission of the one is impossible without the other.

The so-called 'reducing divisions' probably have nothing but a passing and purely adaptive physiological significance in every ontogeny of ova and sperms. The far-fetched and extraordinary teleological significance given by some to the reducing divisions would lead one to suppose that the clairvoyant wisdom of the original egg that thus first threw out the excess of its ancestral 'germ-plasm' in order to save its posterity from harm through the fatality of reversion thus entailed was greater than anything human, if not god-like. The complete parallelism of the 'reducing division' in the sperm and egg has never been established. The comparison of these processes in the two is still only approximate, because in the truly holoblastic egg there is, in some cases, an apparent temporary substitution of the male nucleus for the female, as is shown by the former's assuming a position of equilibrium at the center of the ovum (*Ascaris*), a condition of things that does not and could not occur in the sperm cell.

A still more important contrast is the almost incredible difference of volume of the two kinds of sex-cells of the same species. In man the ratio of volume of the male cell to the female cell is as 1 to 3,000 approximately. This extreme contrast of volume is associated with corresponding contrasts in their properties. There can hardly be any doubt that the mature male cell is in a nearly potential or static state of metabolic transformation of its substance, and is characterized by an almost complete want of stored metabolizable reserve material. The egg is in a similar static state, but, on the other hand, contrasts with the male element in that the development of a more or less voluminous mass of reserve material within it has seemingly been also associated with its loss, as a rule, of the power to begin an independent development. The power of the male cell to begin its transformation and growth through metabolism appears to be arrested until it finds the material in which its mere presence will set up transformations. This it must do by in some way setting free and diffusing some of its own molecules osmotically and mechanically through the egg. The substance of the egg appears therefore to be complementary to that of the spermatozoön. The power to set up transformations within the egg leading to the development of a new being is not manifested aside from the presence of the male element except in cases of parthenogenesis. Even the expulsion of the polar cells is not initiated until the stimulus of the presence of the male element is experienced by the egg.

Another contrast is found in the times of the advent of the 'reducing division' in the two kinds of sex-cells. In the male cell the 'reducing division' occurs earliest, or while it is still in more or less close nutri-

tive relation to the parent; in the egg the 'reducing division' or expulsion of polar cells does not occur till the egg is freed, as a rule, from the parent gonad, and generally as a consequence of the stimulating effect of the presence of the male cell. These differences of behavior of the two sorts of sex-cells seem to be correlated with their differences in size.

We may contemplate the sex-cells as molecular mechanisms which, in virtue of their mechanical structure, are rendered capable of controlling the order and manner of rearrangement of their constituent molecules, because of the new successive attractions and repulsions set free, amongst the latter, immediately upon the completion of conjugation. The new forms of metabolism thus initiated enable us to conceive a mechanical theory of fertilization. At any rate, the two sorts of sex-cells are potentially the reciprocals of each other, and their initial or *statical* states cannot begin to set free their energy and thus pass into the successive kinetic states of formal change until the two mechanisms are reciprocally and mechanically integrated into a single one by means of conjugation. The parts of this new single body now act in unison. Even the manner in which the two conjoined molecular mechanisms operate can actually be to some extent traced, as expressed in the complex movements associated with fertilization, the division of the chromosomes and centrosomes. The effect of conjugation is to afford opportunity also for new and various combinations of molecular mechanisms, though the reciprocal integration of pairs of cells having a widely different parentage.

The great size of the egg-cell provides an extensive reserve material that enables the embryo thus built up usually to reach a relatively great size without entering for a time into competition for food in the struggle for existence. Sexuality is therefore altruistic in nature, since it has led in both plants and animals to the evolution of a condition of endowment, or the storage of potential energy in the germ, so that the latter is the better able to cope with natural conditions. While it may be assumed that sexuality has arisen, in the main, under conditions determined by natural selection, once sexuality was attained, the added power thus accumulated potentially in large germs of double origin enabled the latter the more easily to overcome untoward natural conditions. Natural selection thus becomes altruistic or dotational in that it tends through sexuality to defeat the deadliness of the struggle for existence, just as we may also assert that the theory of superposition to which the mechanical theory of development is committed is also finally altruistic. It may be remarked that the greatest mortality of a species, under the conditions of the struggle for existence, also takes place in the egg and embryonic stages, or before organisms can experience acute pain; so that here again we have a result that must materially ameliorate the pains and penalties of the struggle for life.

These details are, however, of minor import for us just now. The important thing to bear in mind is that all of the forces of development are ultimately metabolic in origin, and that the wonderful order and sequence of events in any given ontogeny arise from the transformation or transposition of the parts of a molecular system that also thus increases in bulk by the addition of new matter. The steps of this transformation are mechanically conditioned by dynamical laws with as much unerring certainty of sequence as those that control the motions of the heavenly bodies. The consequence of such a view is that we can thus free our minds of all traces of belief in a theory of preformation. The embryo is not and cannot be preformed in the germ, as

observation and physiological tests prove; nor is such preformation necessary if a mechanical hypothesis is adopted.

JOHN A. RYDER.

(*To be concluded.*)

CURRENT NOTES ON PHYSIOGRAPHY (VIII.)

CROWLEY'S RIDGE.

CROWLEY'S RIDGE, rising above the alluvial lowland of the Mississippi in Missouri and Arkansas, has long been a subject of discussion. Branner (Geol. Surv. Ark., Ann. Rep., 1889, ii., p. xiv.) has suggested that the lowland to the west of the ridge was excavated as an early path of the Mississippi, from which it was diverted into its present course east of the ridge by the Ohio; but it is difficult to understand how the smaller of the two rivers could divert the larger one. A new explanation of the ridge has recently been offered by C. F. Marbut (Proc. Boston Soc. Nat. Hist. xxvi., 1895), to the effect that the ridge is homologous with the Chunnenugga ridge of Alabama, and that it belongs to a family of geographical forms frequently found on coastal plains during the mature stages of their development. These ridges or uplands normally run parallel to the coast line; they mark the outcrops of comparatively resistant strata, dipping toward the coast; they descend inland by a relatively rapid slope, often strong enough to be called an escarpment, towards an inner lowland which has been eroded on an underlying and weaker member of the coastal formations; they descend more gently on the coastal side. The inner lowland is drained by longitudinal streams, which enter transverse streams that cut their way through the ridge or upland on the way to the sea. In a region of uniform uplift all these features of relief and drainage have a regular rectangular system of trends; but where the former shore line or the uplift is irregular the trends will depart more or less from a rectangular towards a curved pattern. Marbut regards Crowley's ridge as a portion of an inland-curving ridge of this kind. The master stream of the region is the Mississippi, which bisects the inland curvature of the ridge. The upland along whose eastern base the Tennessee river flows northward in an adjusted subsequent course forms the eastern part of the curve; while Crowley's ridge forms the western part. The lignitic strata by which the ridge is determined weaken southwestward, and hence the ridge soon disappears in that direction. The lowland west of Crowley's ridge, ascribed by other writers to erosion by the Mississippi, is explained by Marbut as comparable to the lowland on the inland side of the Chunnenugga ridge of Alabama, and the rivers which follow this lowland are thought to be adjusted subsequent rivers.

THE CUSPATE CAPES OF THE CAROLINA COAST.

THE systematic repetition of certain features in Capes Hatteras, Lookout and Fear is explained by C. Abbe, Jr. (Proc. Boston Soc. Nat. Hist., xxvi., 1895) as the result of a number of backset eddying currents, turning from right to left between the Gulf Stream and the coast. The generally southward movment of the sands along the shore being well known, some special explanation is needed for the acutely pointed capes between the smooth concave curves of the sand bars. Although this is a conspicuous feature of the coast, it seems to have been little considered. Shaler, in his recent general account of Harbors (U. S. Geol. Survey, 13th Ann. Rept., 1893, 180), suggests that the greater inflow of the tides in the middle of the curved bays between the capes would cause a lateral current in either direction, and that the cusps would form where the outward flow from two curves became confluent; but this is contradicted not only by the general southward movement of sands along the shore, but also

by certain minor features to which Abbe gives special attention, and which indicate an outward movement of the prevailing currents on the north side of each cape, but an inward movement on the south side. The V-shaped bars on the shore of ancient Bonneville (Monogr. I., U. S. Geol. Survey, 57) seem to correspond with the cuspate capes in essential features, but their relation to eddying currents is not clearly brought foward by Gilbert. Penck, in his recent *Morphologie der Erdoberfläche*, mentions back-set shore currents as of frequent occurrence, and suggests that the V-shaped bars of the Bonneville shore may have been produced by such movements (II., 485, 486), but he does not refer to other examples of this kind. Yet cuspate sand-bar capes of moderate size are certainly not rare, as may be seen by consulting the maps of our coast in the lower part of Chesapeake Bay.

Dungeness, on the southeastern coast of England, seems to be a similar form; but no other examples are known of so great a size as those of our Carolina coast, nor has any other instance been adduced of so pronounced a control exerted by the general oceanic circulation upon the form of the continental shore line.

THE MIGRATION OF CAPE CANAVERAL.

IN connection with the foregoing, mention may be made of the southward migration of Cape Canaveral, as indicated by the Coast Survey Charts (Nos. XIII., and 159–163). Like the capes further north, Canaveral is a sand-bar cusp, the details of its form indicating a control by two adjacent eddying currents, after the manner described by Abbe. Its history appears to have been in brief as follows: The position taken by the first blunt cusp between the adjacent eddies seems to have been about ten miles south of Mosquito inlet and forty miles north of the present cape; this being, as it were, a provisional location adopted by the currents before much work had been done in shaping the coast by building long bars for the transportation of sand. As an improved and continuous bar grew from north to south, its relation to the general curvature of the Carolina bight was such that it ran past the first-formed cape, and a new location for the cusp was then chosen thirty miles farther south, the outline of the old cape being still faintly traceable inside the newer bar. But a still better adjustment of the currents to the shore brought another bar down from the north, this one running past the apex of the second cape in much the same way that the second bar ran past the first cape; and thus the third cusp, the present Canaveral, was formed ten miles south of the second. The southward migration of the cape appears to be still continued, as indicated by the arrangement of the sand dunes; but it is now going on with a slowly progressive, creeping advance, and not by a leap, such as that which shifted the second cape from the first, or the third from the second. All this, however, is based only on a study of the charts. Those who have opportunity for a study of the cape on the ground might make it the subject of fruitful observation.

W. M. DAVIS.
HARVARD UNIVERSITY.

ANNUAL MEETING OF THE CHEMICAL SOCIETY (LONDON).

IN the course of his address at the anniversary meeting of the Chemical Society of London, the President, Professor Armstrong, after referring to the notable growth of the Society in the twenty years during which he had been a member, stated that the Council had decided to break through the practice which had always obtained and by which the Faraday Lectureship has invariably been filled by some foreign scientist, and had bestowed the Faraday Medal upon Lord Rayleigh 'in recognition of the

services rendered to chemical science by the discovery of argon.' The President added that the Medalist would address the Society on the subject of argon.

Lord Rayleigh said that, in returning his thanks to the Society, he was somewhat embarrassed, because he felt that there ought to be another standing at his side. It was true that his researches, to which the President had referred, upon the densities of gases had rendered it almost certain that a new gas of some sort was concerned, and probably that the new gas was in the atmosphere. But from this point to the isolation and examination of argon was a long step, and the credit must be shared equally between Professor Ramsay and himself. In some quarters there had been a tendency to represent that antagonism existed between chemists and physicists in the matter, though such a thought never entered his mind. Professor Ramsay was a chemist by profession, while he himself had dabbled in chemistry from an early age, and had followed its development with a keen interest.

During the course of the same meeting Professor Ramsay and Mr. Crookes spoke of the isolation and spectroscopic examination of the gas containing helium derived from clèveite.

At the anniversary dinner in the evening of the same day the principal address was made by the Rt. Hon. A. J. Balfour. The following extracts from this will be of interest. Speaking of the attitude of the statesman towards science, he said: "For my own part, though the last thing I wish to do is to suggest that the work of a practical politician is other than a work which taxes the highest qualities of a man, still I have to admit, on looking back at the history of civilization, that if we want to isolate the causes which more than any other conduce to the movements of great civilized societies, you must not look to the great politician of the hour, on whom it may be all eyes are fixed; you must look to those, often unknown by the multitude, whose work, it may be, is never properly realized by the mass of their countrymen till after they are dead. You must look at them, and at their labors, to find the great sources of social movement. We, who are carrying on a work which I hope is not useless, which, I am sure, receives its full meed of public recognition, do, after all, not belong to that class to which the community is most beholden for all that is to improve the lot of man upon earth. It is to those who, very often with no special practical object in view, casting their eyes upon no other object than the abstract truth and the pure truth which it is their desire to elucidate, pen-penetrate ever further and further into the secrets of Nature and provide the practical man with the material upon which he works. Those are the men who, if you analyse the social forces to their ultimate units, those are the men to whom we owe most, and to such men, and to produce such men, and to honor such men, and to educate such men, the Society whose health I am now proposing devotes its best energies. * * *

"I should like to do what I can to dispel the prejudice which certainly exists at this moment in many influential quarters against technical education properly understood. Technical education, properly understood, suffers greatly under technical education improperly understood, and there is so much nonsense talked upon this subject; there is so much money uselessly spent; there are so many things taught to persons who do not want to learn them and who, if they did want to learn them, could by no possibility turn them to practical account; that it is no matter of astonishment that some persons are disposed to say that 'technical education is only the last bit of political humbug, the last new scheme for turning out a brand new society; it is worth-

less in itself; not only is it worthless, but it is excessively expensive.' I am sure Mr. Bryce * would agree with everything I have said upon this point, and everything I am going to say upon it—for I shall not go into controversial matter—because, while I think that those who object to technical education have their justification, it yet remains true that if you include, as you ought to include, within the term technical education the really scientific instruction in the way of turning scientific discoveries to practical account, if that is what you mean—and it is what you ought to mean by technical education—then there is nothing of which England is at this moment in greater need. There is nothing which, if she, in her folly, determines to neglect, will more conduce to the success of her rivals in the markets of the world, and to her inevitable abdication of the position of commercial supremacy which she has hitherto held."

"I do not deny that, if manufactures and commerce have an immense amount to gain from theoretical investigations, on the other hand—as everybody will admit that has even the most cursory acquaintance, let us say, with the history of the discoveries in electricity and magnetism—pure science itself has an enormous amount to gain from industrial development. While both these things are true, I am the last person to deny that it is a poor end, a poor object, for a man of science to look forward to, merely to make money for himself or for other people. After all, while the effect of science on the world is almost incalculable, that effect can only be gained in the future, as it has only been gained in the past, by men of science pursuing knowledge for the sake of knowledge, and for the sake of knowledge alone; and if I thought that by anything that had dropped from me to-night I had given ground for the idea that I looked at science from what is commonly called the strictly utilitarian standpoint—that I measured its triumphs by the number of successful companies it had succeeded in starting, or in the amount of dividends which it gave to the capitalist, or even by the amount of additional comfort which it gave to the masses of the population—I should greatly understate my thought; but I know this great Society, while it has in view these useful objects, still puts first of all the pursuit of truth, which is the goddess to which every man of science owes his devotion. And truth, not profit, must necessarily be the motto of every body of scientific men who desire to be remembered by posterity for their discoveries. These things can only be done through a disinterested motive, and it is because I believe that societies like the great Society I am addressing do more than any other organization to attain that great object; because I think they bring together men engaged in congenial pursuits; because the stimulus of mind brought close to mind, and the honorable ambitions and the honorable rivalries of men engaged in the same great task must lead to an enormous extension of our knowledge of the secrets of Nature; that I, as an outsider, not belonging to your body, do, in the name of a public for which I venture to speak, wish you all success and wish you all prosperity." W. W. R.

* The Rt. Hon. James Bryce, President of the Board of Trade.

CORRESPONDENCE.

HAECKEL'S MONISM.

EDITOR OF SCIENCE: In reponse to your kind note of recent date concerning Haeckle's 'Monistic Creed,' I may state that I find myself in the fullest sympathy with the views expressed by Professor Brooks.

I may perhaps be permitted to add the following:—

The senses of man, as of other animals, yield certain impressions which so far as they go are of the nature of truth. We

know truth only through approximation, the revision and extension of these sense impressions. These impressions and the inductions from them serve as guides to action. In this relation these common impressions must be true, because trust in them has been safe. Wrong action must have led to the destruction of the actors. One test of truth, perhaps the only one, is the safety that comes from trusting it. The power of choice implies that right choice must be made. Only those who in the narrow range of choice choose safely can survive. To this end of safe choice, sensations, desires and reason must coöperate. The adaptation to complex conditions rests on the ability of the individual to receive the degree of truth he needs to make safe choice possible, and no more. For truthfulness in sensation exists only in the range within which action and choice are dependent on it. Beyond this range truth would have no value as an aid to adaptation. Our senses tell us something of truth as to bread and fruit and stones, which we may use or touch or avoid. They do not give us just impressions of the stars or sky, which we cannot reach, nor of the molecule, which we cannot grasp. Our sense powers, as well as our powers of reasoning, are eminently practical. They are bounded by the needs of the lives of our ancestors, to whom any form of *hyperæsthesia* would have been destructive and not helpful.

The methods and the appliances of science serve as an extension of the truthfulness of the senses into regions in which truth was not demanded for the life-purposes of our ancestors. These methods yield truth of a similar kind, which can be measured by the same test. We may trust the information given by the electrometer or the microscope or the calculus just as implicitly as we receive what our own eyes have seen or our own hands have felt. We may depend on the truth given by these instruments of precision to a greater degree than on that which the common senses furnish us, because the guards and checks on scientific appliances are more perfect. The information gained by observation and sifted by reason constitutes science. In the struggle for existence, knowledge is power. Our civilization rests directly on the growth of scientific knowledge and on the availability to the individual of its accumulated power. Its basis is the safety of trusting to human experience. The 'Laws of Nature,' as we know them, are generalizations of such experience. Their statement may form part of a 'scientific creed' to those who have tested them, if such feel that 'I believe' adds force to 'I know.'

The essence of the 'Monistic Creed' as set forth by Haeckel is not, as I understand it, drawn from such sources. It is an outgrowth from Haeckel's personality, not from his researches. So far as I know, no change has taken place in it as a result of any discovery its author has made. If its details have been changed at any time since it was first formulated, the reason for such change must be sought for in Haeckel, not in Science.

Perhaps, indeed, there is "one spirit in all things, and the whole cognizable world is constituted and has been developed in accordance with one fundamental law." But this is no conclusion of science. It rests on no human experience. If it be the induction resulting from all human experience, that fact has not been made plain to us. The hyperæsthesia of the microscope or the Calculus brings one no nearer to it. Its place is in the boundless realm of guesswork. It value lies in the stimulus which clever guesses give to the otherwise plodding operations of scientific men. It seems to me that 'Monism' belongs to the domain of speculative philosophy, a branch of thought which, according to Helmholtz, deals with such 'schlechtes stoff;' that its

conclusions, however brilliant, can have no value as guides to life or as guides to research, which is the second power of life. The theory of Monism has no interest to Science, until men can come to deal with the 'Stoff' on which its speculations rest. Every conceivable theory of life, its nature, origin and destiny, can be traced back to the pre-scientific philosophy of the Ancients, Monism with the rest. What we have found to be true was not unknown to the Greeks. But that which we find to be false had equally the weight of their authority. It is the business of Science to test by its own methods the value of the supposed basis of these theories. The use of logic is one of these methods. The only logical necessity Science can recognize, as Dr. Brooks has well said, is "that when our knowledge ends we should confess our ignorance."

I have myself not the slightest objection to 'Monism' as philosophy. As a dogma it is certainly more attractive than many others which have been brought like lightning from the clouds, as a stimulus to creeping humanity. My objection lies against the use of the divining rod in connection with the microscope. These instruments do not yield homologous results. If both yield Truth, then Truth is a word of double meaning. This method seems to carry us back to the days when truths were made known to the spirit without the intervention of the body. When some theologian of the past brought to Luther the revelations his spirit made to him, the sturdy Reformer said, "Ihren Geist haue ich über die Schnautze" (I slap your spirit on the snout). Scientific men may have as individuals their own visions and guesses and formulæ of Universal Philosophy. Spiritual gymnastics are not without value to any worker, and men of science have often suffered from their neglect. But this suffering is purely individual. The running high jump does not hasten the progress of knowledge. Science will have none of it. Nor will she tolerate a divining rod even in the hands of her wisest devotees. In other words, where the facts stop Science stops also.

DAVID STARR JORDAN.

STANFORD UNIVERSITY.

THE GENUS ZAGLOSSUS.

TO THE EDITOR OF SCIENCE: Mr. T. S. Palmer's article in SCIENCE of May 10th fixes the synonymy of this genus with precision; but one statement he makes is incorrect, namely, that '*Zaglossus* Gill seems never to have been mentioned by any subsequent author.' The Century Dictionary has three articles from my pen on the subject. 1. *Zaglossus* is defined as 'the proper name of that genus of prickly ant-eaters which is better known by its synonym *Acanthoglossus* (which see).' 2. Under *Acanthoglossus* the genus is characterized, with the statement that this name 'is antedated by *Zaglossus* of Gill.' 3. Under *Echidnidæ* the animal is figured with the legend '*Zaglossus* or *Acanthoglossus bruijni*.'

ELLIOTT COUES.

SCIENTIFIC LITERATURE.

The Cambridge Natural History, III., Molluscs: By the REV. A. H. COOK; *Brachiopods* (Recent): By A. E. SHIPLEY; *Brachiopods* (Fossil): By F. R. C. REED. New York, Macmillan & Co. 1895. XIV., 536. Pp. 8°. Illustrated.

This work is one of a series intended especially for intelligent persons without scientific training, but in which the attempt is made to combine popular treatment and untechnical language with the latest results of scientific research.

Mr. Cooke, who is known as a painstaking and well informed conchologist, has endeavored to unite in one general classification the views of specialists in the various groups, such as Hoyle for the recent, Foord and Fischer for the fossil Cephalopods, Bergh for the Nudibranches, Pelseneer for the Pelecypoda, etc.; but, in conformity with

the general purpose of the work, much more space is devoted to the geographical distribution and general natural history of mollusks than to the details of systematic arrangement or technical discussion. Twelve chapters of 377 pages are devoted to generalities, and four, comprising 66 pages, to classification.

The work deserves high commendation for the thorough manner in which Mr. Cooke has foraged for fresh data, bringing together a vast number of facts on the biography, distribution, growth, anatomy and reproduction of mollusks. The style is clear and easy, and the facts are well selected and agreeably presented. For the audience for which the book is intended it seems admirably adapted, and so far as we know there is no work available at present which can be more cordially recommended to a beginner or the general reader.

It would be easy to criticise details of classification here and there, and on many points the opinions of experts will differ in the present state of our knowledge; but in recognizing the aim of the author and publishers it must be conceded that it has been well carried out.

It does not appear to have been necessary to separate the recent from the fossil brachiopoda, and recent efforts at a revised classification of the group have been so successful and complete that Mr. Reed's work appears already somewhat antiquated and too brief, but this perhaps was inevitable from the necessity of preserving due proportion between the parts of the series. Mr. Shipley's account of the anatomy and embryology is good, and his conclusions as to the relations of the class are conservative and reasonable.

The book is fully illustrated with rather unequal woodcuts, many of which are good and others rather 'wooden,' but an unusually large proportion of them are original and fresh. There are four very good maps of geographical distribution and an excellent index. W. H. DALL.

A Laboratory Guide for a Twenty Weeks' Course in General Chemistry. By GEORGE WILLARD BENTON, A. M., Instructor of Chemistry, High School, and Chemist for the City of Indianapolis. Boston, D. C. Heath & Co.

This book might be better termed 'A Guide for a Course of Test-Tubing,' since nearly all the reactions are performed in a test-tube, and the sole object of the book seems to be to acquaint the unfortunate pupil who uses it with 'Tests' for the various elements and compounds.

The manual is supposed to be put into the hands of beginners in the subject, and yet before a single element is considered or anything is said about elements, compounds or formulas, quite a number of formulas and reactions are given. As an illustration of what the author calls compounds, a piece of wood and granulated sugar are taken and the equation $C_{12}H_{22}O_{11} + H_2SO_4 = 12C + 11H_2O + H_2SO_4$, is written out. Then the student is asked to explain the equation and to define a compound. And yet the author, according to his preface, is one of those 'who see in the Laboratory (with a big L) the means of high development on approved pedagogical grounds.'

It would require more space than the book is worth to point out all its faults. It will, perhaps, be sufficient to state that directions are given for making dangerous compounds without any mention of the danger connected with the work. The pupil is asked, for example, to determine the odor of carbon monoxide, and not an intimation is given that it is one of the most poisonous gases known to the chemist.

Altogether, the book is one that can be most cordially recommended as the kind of a book for both teachers and students to avoid using, if possible. W. R. O.

NOTES AND NEWS.

THE HELMHOLTZ MEMORIAL.

THE following subscriptions have been paid to Prof. Hugo Münsterberg, Secretary and Treasurer of the American Committee, Cambridge, Mass. Further subscriptions should be sent to him at an early date :

A. Agassiz, Cambridge,	$ 25
S. P. Avery, New York,	10
Clarence J. Blake, Boston,	20
Francis Blake, Boston,	50
H. P. Bowditch, Boston,	20
W. N. Bullard, Boston,	10
J. McK. Cattell, New York,	5
F. A. Christie, Meadville, Pa.,	2
Clarence N. Clark, Philadelphia,	25
A. Donner, Boston,	25
Mrs. M. A. P. Draper, New York,	50
W. G. Farlow, Cambridge,	5
H. N. Gardiner, Northampton,	3
E. Grüning, New York,	15
C. C. Harrison, Philadelphia,	100
A. Jacobi, New York,	10
W. James, Cambridge,	5
J. Jeffries, Boston,	5
H. Knapp, New York,	100
Seth Low, New York,	100
Oswald Ottendorfer, New York,	200
E. C. Pickering, Cambridge,	20
J. J. Putnam, Boston,	5
W. A. S., New York,	10
G. de Schweinitz, Philadelphia,	25
N. S. Shaler, Cambridge,	5
Society of Eye Surgeons, San Francisco,	25
D. P. Todd, Amherst, Mass.,	10
O. F. Wadsworth, Boston,	20
H. C. Warren, Cambridge,	25
D. Webster, New York,	5
Henry W. Williams, Boston,	25
N. Wilmer, Washington,	10
Total,	$970

THE GEOLOGICAL SOCIETY OF AMERICA.

THE Geological Society of America will hold its seventh Summer Meeting at Springfield, Mass., Tuesday and Wednesday, August 27 and 28. The Council will meet Monday evening and the Society will convene Tuesday morning at 10 o'clock.

The Fellowship of this Society includes nearly all the working geologists upon the continent. The roll now contains 223 names of Fellows.

The former Presidents of the Society have been James Hall, James D. Dana, Alexander Winchell, G. K. Gilbert, J. William Dawson and T. C. Chamberlin.

The officers for 1895 are as follows:

President, N. S. Shaler, Harvard University.

Vice-Presidents, Joseph Le Conte, University of California; Charles H. Hitchcock, Dartmouth College.

Secretary, H. L. Fairchild, University of Rochester.

Treasurer, I. C. White, Morgantown, W. Va.

Editor, J. Stanley-Brown, Washington, D. C.

Councillors:

F. D. Adams, McGill College, Montreal.
R. W. Ells, Geological Survey of Canada.
I. C. Russell, University of Michigan.
E. A. Smith, University of Alabama.
C. R. Van Hise, University of Wisconsin.
C. D. Walcott, U. S. Geological Survey.

The Society has just completed the sixth volume of its Bulletin, which is a handsome octavo, with 528 pages and 27 plates. This volume includes twenty-one brochures.

Information concerning the Society and its publications can be obtained by addressing the Secretary, H. L. Fairchild, Rochester, N. Y.

NOMINATIONS BEFORE THE ROYAL SOCIETY.

THE following fifteen candidates were selected by the Council of the Royal Society to be recommended for election into the Society: J. Wolfe Barry, civil engineer, Vice-President of the Institution of Civil

Engineers; Alfred Gibbs Bourne, Professor of Biology in the Presidency College, Madras; George Hartley Bryan, Fellow of Peterhouse, Cambridge, and Lecturer on Thermodynamics on the University list; John Eliot, Meteorological Reporter to the Government of India; Joseph Reynolds Green, Professor of Botany in the Pharmaceutical Society of Great Britain; Ernest Howard Griffiths, physicist Private Tutor; Charles Thomas Heycock, Lecturer on Natural Science, King's College, Cambridge; Sydney John Hickson, biologist, Fellow of Downing College, Cambridge; Henry Capel Lofft Holden, Major Royal Artillery, electrician; Frank McClean, astronomer; William Mac Ewan, Professor of Surgery, University of Glasgow; Sidney Martin, Assistant Physician, University College Hospital and Hospital for Consumption, Brompton; George M. Minchin, Professor of Mathematics in the Royal Engineering College, Cooper's Hill; William Henry Power, Assistant Medical Officer, H. M. Local Government Board; Thomas Purdie, Professor of Chemistry in the University of St. Andrews.

JOHN A. RYDER.

A JOINT meeting of members of the University of Pennsylvania, the American Philosophical Society and the Academy of Natural Sciences was held in the hall of the Academy of Natural Sciences on the evening of Wednesday, April 10, in memory of the late Professor John A. Ryder. General Isaac J. Wistar presided and Philip P. Calvert acted as secretary. Addresses were made by Dr. Harrison Allen on 'Dr. Ryder's Relation to the Academy of Natural Sciences;' Dr. Bashford Dean, of Columbia College, on 'Dr. Ryder's Work in the U. S. Fish Commission'; Dr. Horace Jayne, on 'Dr. Ryder and the School of Biology'; Prof. E. D. Cope, on 'The Evolutionary Doctrine of Dr. Ryder;' Dr. H. F. Moore, on 'Dr. Ryder as a Teacher,' and Dr. W. P. Wilson, on 'Dr. Ryder as a Collegian.' The speakers all bore testimony to Professor Ryder's merits as an investigator and as a teacher and to his amiability and honesty as a man.—*American Naturalist.*

GENERAL.

THE *Gesellschaft für Erdkunde* at Berlin has just issued the first volume of a bibliography of geographical science entitled Biblioteca Geographica, edited by Otto Baschin with the assistance of Dr. Ernst Wagner. The volume covers 1891 and 1892 and the society proposes to continue the publication annually. The scope of the work is in full accord with the widest understanding of the word geography. The editor, Otto Baschin, Berlin, W. Schinkenplatz 6, requests that authors send titles and works relating to geography to him.

THE *Imprimerie Polytechnique* at Brussels announces an important Egyptological work by G. Hagemans, which will include a history of Egyptian civilization, a summary of Egyptian literature and a discussion of the Egyptian writing, including a comparison between its hieroglyphs and those of Yucatan; this is to be followed by a Copto-Egyptian grammar, an Egyptian-French and a French-Egyptian dictionary. The entire work will appear in sixty parts at 25 cents per part.

WE learn from *La Nature* that at the annual meeting of '*Le Congrès des Sociétés Savantes*' at the Sorbonne, Paris, on April 20th, under the presidency of M. Poincaré, M. Moissan called attention to the rapid progress and brilliant discoveries of modern chemistry, and their practical outcome in stimulating national industries. He passed under review the processes of manufacturing iron, steel, aluminium, etc., the artificial production of the diamond, the crystalization of metallic oxides, and the use of electricity in the decomposition of those oxides hitherto regarded as irreducible. At the

close of the meeting M. Poincaré was elected president for a second term. The Legion of Honor was conferred on MM. le comte d'Avenal, O'Ehlert and Herluison.

The honorary degree of D. Sc. has been conferred on Mr. Francis Galton by the University of Cambridge.

The statute establishing degrees for research at Oxford has now been finally approved by Congregation, with the adoption of several amendments, principally of a technical nature.

The University of Aberdeen is about to confer the degree LL. D. on Miss J. E. Harrison in recognition of her researches in Greek archæology. Miss Harrison will be the first woman to receive this degree from a British university.

Dr. Richard Hanitsch, demonstrator of zoölogy at University College, Liverpool, has been appointed to the curatorship of the Raffles Museum, at Singapore.

The *Evening Post* states that the Herbarium of Rousseau, composed of fifteen quarto volumes in cardboard and containing about 1,500 plants, is about to be sold at Orleans.

At a recent sale in London, Gilbert White's Natural History of Selborne, the author's original manuscript, in the form of letters to Thomas Pennant and Daines Barrington, first printed in 1789, was sold for £294. The manuscript contains many passages not printed in the several editions, and has never before been out of the possession of the lineal descendants of the author.

A catalogue of the Philosophical Transactions of the Royal Society from 1824 to 1893 has been issued by Dulau & Co., London. A large number of separate articles are included. Especially worthy of note is a paper on 'Observations on the Parallel Roads of Glen Roy * * * with an attempt to prove that they are of Marine Origin' (1839), by Darwin, as also articles by Sir Humphrey Davy, William and Sir F. Herschell, Sir E. Sabine, Sir David Brewster, Faraday, Sir Richard Owen and Cayley.

Mr. Arthur M. Wellington, the well known engineer, died in New York at the age of forty-eight.

Prof. E. Ray Lankester is giving a course of four lectures at the Royal Institution on 'Thirty Years' Progress in Biological Science.'

Mrs. Robert E. Peary delivered an illustrated lecture based on her experiences in the North on May 23. This lecture was given under the auspices of the National Geographic Society, which aided Lieut. Peary in his first enterprise. The proceeds of the lecture will be devoted to a fund which is being raised to defray the expenses of an expedition that will enable Lieut. Peary to return to America. It is not believed, however, that he is in any immediate danger. The expedition (which will cost from $9,000 to $12,000, of which about $7, 000 has already been raised) will probably start about July 5th, so as to reach Lieut. Peary's headquarters before September 1st.

At the meeting of the Boston Scientific Society, was held on May 28th, an address on 'Some Problems in the Use of Water Power as Applied to the Electrical Transmission of Power' was delivered by Allan V. Garratt.

Professor Dyche, of Kansas University, is starting for Greenland in search of specimens of mammals and birds to add to his collection.

Chancellor James Hulme Canfield has accepted a call to the presidency of the Ohio State University, Columbus.

An infirmary in connection with Harvard University, which is proposed as a memorial to Dr. Peabody, is projected, costing not less than $12,000. President Eliot, in the name of the overseers of Harvard University, has offered a site for the infirmary,

providing the money to build it can be raised.

DR. JAMES E. RUSSELL has been made professor of pedagogy in the University of Colorado.

THE American Institute of Archæology, which had already given a fellowship of $600 to the American school at Athens, voted a second fellowship of $600–$800 at the semi-annual meeting of the committee held at Middletown, Conn., on May 17th. These scholarships will probably be awarded to students and graduates of the coöperating colleges on competitive examination. The first examination will probably be held at the end of a year.

PROF. E. S. HOLDEN has been made a commander of the Order of the Ernestine House of Saxony in recognition of his services to science.

DR. P. DANGEARD has been appointed professor of botany to the Faculty of Sciences at Poitiers.—*Nature*.

WE learn from the *Naturwissenschaftliche Rundschau* that Prof. Overbeck of Greifswald has been appointed professor of physics in the University of Tübingen as successor to Professor Braun. Dr. Hermann Struve, astronomer in the Observatory of Pulkowa, has been made professor of astronomy in the University of Königsberg; Prof. Koken of Königsberg, professor of geology and mineralogy in Tübingen; Prof. Hauser of Erlangen, Director of the Erlangen Anatomical Institution; Prof. Brauns of Karlsruhe, professor of geology and mineralogy in Giessen, and Dr. Schutt of Kiel, professor of botany in the University of Greifswald.

PROFESSOR V. KNORRE has been called to the new chair of electro-chemistry in the technical High School at Berlin-Charlottenburg.

THE death is announced on May 4th of Surgeon-Major Carter, F. R. S., also of Prof. Manuel Pinheiro Chagas, General Secretary of the Royal Academy of Sciences at Lisbon, at the age of fifty-three.

IT is announced that Dr. J. P. D. John, who resigned the presidency of De Pauw University a few days ago, will be asked by the trustees to reconsider his resignation.—*Evening Post*.

THEOBALD SMITH, M. D., has been elected professor of applied zoölogy, and Henry Lloyd Smythe assistant professor of mining, in Harvard University.

AT the semi-annual meeting of the trustees of the American University it was announced that $127,300 had been subscribed towards the erection of the first building (the Hall of History), but that $150,000 were required. Those present at the meeting subscribed and assumed the entire deficiency.

DR. ROB. SACHSSE, assistant professor of agricultural chemistry in Leipzig University, died on April 26.

SCIENTIFIC JOURNALS.

THE ASTROPHYSICAL JOURNAL, MAY.

The Modern Spectroscope, XII: WILLIAM HUGGINS.

Dr. Huggins here describes the Tulse Hill ultra-violet spectroscope. An earlier arrangement of telescope and spectroscope had consisted in exchanging the small mirror of an eighteen-inch Cassegrain telescope for a spectroscope with its slit in the principal focus of the large mirror. Difficulties of adjustment and the sacrifice of either light or purity due to the restricted size of the spectroscope led to the abandonment of this form. The small speculum was replaced and the collimator was then inserted in the hole through the large mirror. The long equivalent focal length of the Cassegrain form is of advantage where it is desirable to have images of considerable dimensions upon the slit, while the instrument itself and the building may remain of moderate size.

On the Spectrographic Performance of the Thirty-inch Pulkowa Refractor: A. BÉLOPOLSKY.

The work of the great refractor with a spectrograph not well adapted to it compares unfavorably with that of the new thirteen-inch photographic telescope.

Note on the Spectrum of Argon: H. F. NEWALL.

A line spectrum obtained last year under peculiar conditions of low pressure has been identified as that of argon. A glass bulb was sealed to a mercury pump and the air exhausted. Two photographs, with an exposure for each of thirty minutes, differed in that the second showed the nitrogen bands much weaker than the first, besides containing lines since identified as those of argon.

Preliminary Table of Solar Spectrum Wave-Lengths, V: HENRY A. ROWLAND.

The table is continued from λ 4414 to λ 4674.

On Martian Longitudes: PERCIVAL LOWELL.

A series of observations on the positions of thirty-six points on Mars with a view to the construction of a map. A discrepancy of five degrees between present longitudes and those determined by Schiaparelli in 1879 suggests that the received time of rotation of the planet is too small.

A Combination Telescope and Dome: A. E. DOUGLASS.

The article describes a novel plan of mounting a telescope within a hollow sphere supported like an ordinary globe, but with much of the weight taken off from the supports by floating the sphere in water. The plan is the result of an effort to reduce the instability of the usual mounting by flotation, and the application of the motive power as far as possible from the axes of rotation.

Stars Having Peculiar Spectra; Eleven New Variable Stars: M. FLEMING.

Some Arequipa photographs show eleven peculiar star spectra and eleven new variables.

A Spectroscopic Proof of the Meteoric Constitution of Saturn's Rings: JAMES E. KEELER.

The spectrum of the planet was photographed with the slit parallel to the major axis of the rings. The inclination of the spectral lines of the ansæ show that the inner part of the ring is moving faster than the outer portion, which would not be the case were the rings moving as a solid. The indicated velocities of the different parts satisfy Kepler's third law.

Remarks on Professor Pickering's 'Comparison of Photometric Magnitudes of the Stars,' in A. N. 3269: G. MÜLLER and P. KEMPF.

A criticism of the Cambridge catalogues, translated from the Astronomische Nachrichten.

The Short Wave-Lengths of the Spark Spectrum of Aluminium: C. RUNGE.

A Large Eruptive Prominence; On a Photographic Method of Determining the Visibility of Interference Fringes in Spectroscopic Measurements; Note on the Exposure Required in Photographing the Solar Corona Without an Eclipse: GEORGE E. HALE.

Terrestrial Helium (?).

A Large Reflector for the Lick Observatory: EDWARD S. HOLDEN.

S. B. BARRETT.

NEW BOOKS.

The Natural History of Plants; their Forms, Growth, Reproduction and Distribution. From the German of Anton Kerner von Marilawn, by F. W. OLIVER, with the assistance of MARIAN BUSK and MARY F. EWART. With almost 1,000 original wood-cut illustrations and 16 plates in colors. New York, Henry Holt & Co. 1895. 40, Vol. I., in two parts. Pp. 777. Price $7.50.

Twentieth Annual Report of the Secretary of the State Board of Health of the State of Michigan. Lansing, Robert Smith & Co. 1894. Pp. cxlvi + 416.